

introduction of useful mechanical inventions, and the means were to be courses of philosophical lectures and experiments illustrating the applications of science to daily life.

The Chairman, in acknowledgment of the toast, said that it was a great honour that so many eminent representatives of foreign science had honoured with their presence the centenary of the Institution. It was just 100 years ago when the Institution entered upon its present premises. A long roll of names had lent lustre to their labours. Davy, Faraday, Young, Tyndall—above all, they should remember their founder, Benjamin Thomson, Count Rumford, whom it was easy to criticise, but whose virtues had been productive of great results. The work of the Institution had been in large measure the carrying out of Count Rumford's ideas. It was said that he intended an institution of a more practical or industrial character than the Institution now was. But changes had taken place. Facilities for communicating new discoveries were 100 years ago few; competition was less keen; there was then much dislike of innovation, and there was extreme jealousy with the working classes of any reduction of manual labour. It was thus necessary to popularise discoveries; and that was the aim of their founder. But now every such discovery was soon heralded to the public. Popular magazines had now articles on the manufacture of liquid air and other subjects of an abstruse character. Towards this wide diffusion of science the Royal Institution had largely contributed. Their principal objects were research, for which their laboratories gave ample means, and in respect of which special gratitude was due to Dr. Mond for his noble gift, and to Mr. Spottiswoode for his collection. The second object was to bring the results of research to the knowledge of those who could appreciate them, and these results were expounded in the evening lectures of the Institution. Thirdly, this knowledge was popularised by the afternoon lectures; and, finally, the rising generation were stimulated by the juvenile lectures to those who, it was hoped, were destined to take their part in future scientific investigation.

On Tuesday afternoon a commemoration lecture was delivered at the Institution by Lord Rayleigh, the Prince of Wales being present.

In the course of his remarks, Lord Rayleigh is reported by the *Times* to have said that though his was intended to be a commemorative lecture, the idea of commemorating all the work that had been done at the Royal Institution was hopeless. Remembering that on other occasions he had spoken of the achievements of Faraday and Tyndall, he thought on this occasion he would do well to go still further back in the century and speak of Dr. Thomas Young, one of the earliest professors of the Institution. Young occupied a very high place in the estimation of men of science—higher, indeed, now than at the time when he did his work. His "Lectures on Natural Philosophy," containing the substance of courses delivered in the Institution, was a very remarkable book, which was not known as widely as it ought to be. Its expositions in some branches were unexcelled even now, and it contained some things which, so far as he knew, were not to be found elsewhere. The earlier lectures dealt with mechanics, and the reader would find as sound an exposition of that science as could be imagined. Elastic resilience, or what we should now call potential energy, was better dealt with there than in any other treatise he knew, for Young discussed the subject with remarkable ingenuity, showing that the phenomena exhibited by two bodies coming into collision were comprehended under two cases. In the province of sound, Young was the originator of many of the most important principles on which the doctrine was now expounded, but it was with optics that his name was most closely associated, for Fresnel and he were the builders of the great structure of the undulatory theory. Lord Rayleigh then mentioned some of the points in which Young's good work had been overlooked. In Young's time one question of discussion was the change of the focus of the eye for varying distances. One suggested explanation, that accommodation was affected by an alteration in the external convexity of the eye, Young proved to be wrong by drowning his eye in water. This virtually altered the convexity, yet the power of accommodation remained, and he therefore concluded it was due to a muscular alteration in the internal lens of the eye. Young was singularly successful in the theory of cohesion and

capillarity, in which some of his earliest work was done, and he was the first to deduce an estimate of molecular dimensions from data afforded by that theory. The size of the molecule, according to his calculations, was not very different from that admitted at the present day. In the theory of the tides he made great advances, while his views on heat were very interesting, since he had the utmost contempt for the idea prevalent in his time that it was a separate entity, and expressed the hope that in time philosophers might arrive at a true conception of its nature as motion. Speaking of work which had been done at the Institution by men who held no regular appointment in it, the lecturer noted that Wedgwood, in conjunction with Davy, was the first to produce anything that could be called a photograph, while instantaneous photography, such as was required for rapidly moving objects, was carried out for the first time by Fox Talbot in the laboratory of the Institution.

Another commemoration lecture is to be delivered as we go to press. Upon the invitation of the teachers of natural science in Oxford University, honorary members of the Institution will visit the University to-day.

The principal historical apparatus in the Institution has been on view during the centenary celebration. An interesting souvenir of the centenary is an illustrated brochure referring to William Spottiswoode, and to his collection of physical apparatus just presented to the Institution by his son, Mr. W. H. Spottiswoode. The souvenir includes a memoir of Spottiswoode, reprinted from *NATURE* of April 26, 1883; a list of lectures delivered by him at the Royal Institution, notes on some of the more important objects in the collection of apparatus, a reprint of a paper by Spottiswoode on the laboratories of the Institution, and a chronological list of original work developed at the Institution. A photogravure of Spottiswoode, and a number of brilliant half-tone pictures of sets of objects in the collection of apparatus, form part of Mr. Spottiswoode's interesting pamphlet.

#### THE HEIGHT OF THE AURORA.<sup>1</sup>

A GOOD story used to be told some years ago of a candidate, who, when undergoing the torture of a *viva voce* examination, was unable to reply satisfactorily to any of the questions asked. "Come, sir," said the examiner, with the air of a man asking the simplest question, "explain to me the cause of the aurora borealis." "Sir," said the unhappy aspirant for physical honours, "I could have explained it perfectly yesterday, but nervousness has, I think, made me lose my memory." "This is very unfortunate," said the examiner, "you are the only man who could have explained this mystery, and you have forgotten it." One is not prepared to say that exact and complete knowledge of the cause of this curious phenomenon has greatly advanced since the time when the examiner made this crushing rejoinder, and it is therefore fortunate to have to treat of only one of the difficulties with which the whole problem is beset—the height at which the light manifests itself, or the limits of altitude above the earth's surface at which it may be seen. But a preliminary difficulty arises in connection with even this bare statement. Is the aurora borealis a localised phenomenon? Has it a habitation as well as a name? Or is it, like the rainbow, an optical exhibition resulting from the operation of certain physical causes. In the case of the rainbow, the causes admit of a tolerably simple explanation, and little is to be learnt from the study of its general features as seen in the sky; certainly we should not think it betokened any great show of wisdom to attempt to determine its height by any method of measurement or triangulation. The angular altitude is settled for us in a quite different manner, and it may

<sup>1</sup> "The Altitude of the Aurora above the Earth's Surface." By Prof. Cleveland Abbe. ("Terrestrial Magnetism," vol. iii., 1898.

be that we are displaying a crass ignorance in endeavouring to apply to auroræ methods of measurement which depend for their success upon an apparent displacement, due to a real change in the position of the observer. Students of trigonometry are taught at a very early stage the method of determining the distance of an inaccessible object, visible from two positions, and the elementary process employed, depending on the solution of a triangle, remains the favourite method of determining the height of the aurora. But just as our student knows that a successful solution of the problem demands that the angles must refer to a concrete object, so the observer of an aurora asks that this fitful light should have a definite "locus," that can be simultaneously seen and identified by two or more observers. Practically, this fundamental condition is not always easily satisfied, and other methods have in consequence been suggested which are founded on a supposed knowledge of the origin and behaviour of the auroral light. We may say at once that these methods, often ingenious in themselves, are so unsatisfactory in application that they can be passed over with a very brief mention.

There is no doubt but that the light out of which the auroral phenomena are formed emanates from a certain circumscribed region, but the real question is whether the arches and beams, the streamers and waves, the curtains and folds, with all the varied nomenclature that has been used to describe special features, are definite concrete objects. The fact that the aurora is accompanied with a special and presumably constant spectrum, possessing easily recognisable characteristics, does not help us at all to settle the question. That we have a source of light is admitted. The point at issue is, to what extent is it a subjective phenomenon, and how far does each observer see his own aurora as an optical illusion. Manifestly, those who accept the subjective theory have to encounter many objections. No one likes to admit that he is deceived in the character of a phenomenon so apparently real as that presented by a fine auroral display, though he will readily acknowledge that perspective must introduce some misleading features. Prof. Cleveland Abbe has, however, summed up the evidence with great care and completeness, and come to the conclusion that the idea of an individual existence must be definitely relinquished. He has been led to this conclusion from an examination of the various attempts that have been made to determine the height of the aurora; and whether we accept this decision or not, we shall at least be prepared to follow him in the assertion that the determination of the altitude of the aurora is a much more delicate problem, and perhaps also a more indefinite problem, than we have hitherto believed.

The evidence tending to this latter conclusion can be divided under many heads. We shall content ourselves with exhibiting two—one depending upon actual observation and measurement, the other resting upon theory and suggestion. The observers who have made the height of the aurora a special study can be grouped into two families—one represented by Richardson, Franklin, Hooker, and Silberman, who have actually seen the aurora below the clouds, or between themselves and neighbouring objects; and others like Loomis, Boscovich, and Twining, who place the height anywhere between 400 and 1000 miles. Between these advocates for a "ground" theory, and those who perceive a high aerial origin, we have a whole host of observers who are mainly led to their results by the selection and rejection of certain of their observations, if they are numerous, or have drawn their conclusions from single and accidental results. For the statement of claim of those who argue that the aurora is entirely confined to the lowest stratum of the earth's atmosphere, we must trust entirely

to description. Measurement can evidently play no part, any more than it can on a bank of fog or a shower of hail. An admirable description, and one that would carry conviction to every impartial reader, if we could give it fully, has been written by Prof. J. P. Lesley, the distinguished geologist, of what he saw at Little Glace Bay, about seventeen miles from Sydney, Cape Breton: "It was my good fortune to observe an aurora, which to my eyes was embodied in and swept the earth with successive banks of Cape Breton fog. . . . In this fog bank hung, as it were, a brilliant curtain of light, with a wide fringe or flounce of maximum brilliancy, along the bottom edge, the light fading upwards along the curtain, but traceable to the very zenith, and the curtain stretching from the eastern horizon out at sea to the western horizon on the low hill-tops. The perspective was perfect. The curtain was evidently vertical, thin, straight, long enough to reach from one limit of the vision to the other, and floating broadside before the south wind towards the north. No reasoning could convince us (he had a companion) that these were not elements of the phenomenon, and, moreover, that the lower edge of the bright fringe was more than one or two hundred yards away at its nearest point when we first saw it. Its rate of departure from us was evidently that of the fog bank, or that of the gentle south wind then blowing. The perspective of the whole curtain changed in conformity with that supposition. We had both spent our lives in topographical work, and no record of triangulation made upon this aurora would alter my conviction of the posture and movements of the beautiful object, derived from the natural triangulations of the unassisted eye." Prof. Lesley further relates that he witnessed successive repetitions of the same beautiful appearance, but feebler in intensity, as though produced by the same causes gradually growing less and less active, from a process of exhaustion. Of the accuracy of his testimony he can entertain no doubt, and urges that it is unreasonable that the positive observations of those who have witnessed these displays should be despotically overridden by the trigonometrical calculations of other students. We are inclined to agree with him. It might, of course, be urged by those who consider that the upper and attenuated regions of the atmosphere are necessary for the production of auroral light, that such an exhibition was not a true aurora, and that if examined spectroscopically the light would not show the characteristic lines, nor would the magnetic instruments in the neighbourhood be agitated in the manner with which we have been made familiar when auroræ are present. On these points there seems to be no evidence, nor, so far as is known, have other physicists, who, like General Sabine, have "walked through an aurora as one would pass through a mist," verified their convictions.

But if the deductions of those who trust to the evidence of their senses can be set aside as affected by self-deception, others who rely on elaborate measurements are hardly in better case. With more pretentious methods, more rigorous criticism can be applied. Into the details of this criticism it is not convenient to enter here, involving as it does that much-debated quantity the "probable error," and still more recondite criteria for the rejection of discordant observations. But we have a right to expect an intelligible result, and this is not in every case forthcoming. If a man carefully surveyed a field with the view of determining its superficial area, brought out as his result a minus quantity, we should necessarily have a difficulty in explaining his deductions. And, without any exaggeration, it is precisely results of this character which are too frequently obtained from attempted measurements of auroræ. Of course, in the case of an object so ill-defined as an auroral arch, one expects to find large observational errors. But these



errors should have less effect in proportion as the conditions for securing accuracy in the solution of the problem increase. For instance, if we are going to measure a distance of two, four, or six hundred miles, a base of a mile or less in length will give us a very ill-conditioned triangle; but as we increase the length of the base, we should expect greater consistency in the results. Unfortunately this expectation is not realised. As an illustration, we select, out of the mass of measures that Prof. Cleveland Abbe has collected, three series of observations. The earliest of these sets was made in 1839, by MM. Bravais and Lottin in Norway, in latitude about  $+70^\circ$ . The stations selected provided a base line about ten miles in length. Even at this moderate distance, the two expert observers could not recognise the same features in the auroral arch, or be certain that the angles measured with their theodolites referred to the same point. But Bravais, greatly daring, boldly applied trigonometrical methods, and deduced a parallax with mathematical rigour. Out of seven measures, as shown below, three parallaxes are negative and four positive, the total range being more than eleven degrees.

	h. m. ° /	Parallax		h. m. ° /	Parallax
1839 January 12,	5 37 - 3 42		1839 January 21, 6	2 - 1 34	
" " 12,	6 2 + 2 13		" " 21, 7	3 + 1 4	
" " 12,	9 30 + 9 52		" " 21, 7	33 + 0 45	
" " 12,	10 36 - 0 8				

Bravais concluded from all his observations that the mean altitude of the auroral arch is between 100 and 150 kilometres, but suggested that in order to determine "the parallax of the aurora more precisely than we have been able to do, it would be necessary to employ a longer base than ours, say about 100 kilometres in length, and directed as nearly as possible parallel to the vertical plane through the culmination of the arch." Fifty years later, Bravais' suggestion was carried out. Tromholt of Rostock, in Norway, occupied one of the stations near the scene of Bravais' earlier investigations, but extended the base line to a length of 66 miles. From one end of this base line, Tromholt made no less than 634 measurements, while 367 were made from the companion site. On comparing the results, however, only sixty corresponded as to time and referred, or were supposed to refer, to identical objects. These sixty were again reduced to forty-two, for reasons which do not appear; but it would be scarcely uncharitable to suggest that the remainder gave negative or impossible parallaxes. This modest remainder reminds one of Falstaff's "half-pennyworth of bread to his intolerable deal of sack." The final result, however, taken for what it is worth, assigns altitudes to the auroræ varying from 19 to 217 kilometres. Variations so great in amount cannot inspire confidence.

It might, however, be objected that in the Tromholt series, since the observers were separated some 100 km., that only the upper features of the aurora could be visible simultaneously from both stations, and that if the true or localised aurora was confined to the lower strata of the atmosphere, more or less illusory results might be expected. But we have a third series, made in about the same latitude, in which the observers were stationed only about a third of a mile apart, where they were in constant telephonic communication with each other, and where, therefore, the conditions were favourable to the removal of some of the difficulties that beset the parallax method. Without quoting in detail the results obtained, it will be sufficient to say that, on discussion, the number of positive and negative parallaxes, even after judicious rejections, was found to be seventeen and twenty-three respectively, and that consequently no trustworthy value of the height could be deduced. Prof. Cleveland Abbe shows in this particular case how

the calculus of probabilities has been forced in order to derive a plausible altitude from these observations. There is, however, no necessity to labour the point. We are simply concerned to show that the method tried under various conditions fails to give consistent results. Those who believe that the aurora is confined to the upper regions of the atmosphere reject the largest parallaxes, while those who are fighting for a low aurora will only accept the large values. The one fact which seems to stand out clearly after much patient examination is that the parallax does not increase with the increase of the length of the base line, or, in other words, it cannot be a true parallax. There is no dearth of reasons to explain these discrepant results. The inevitable error of observation arising from the feebleness of the light, the want of clear definition at the boundary of the arch, the possible movement of the object itself, and the want of absolute synchronism in the measurements at the stations, would be more than sufficient to make the method untrustworthy.

In presence of these difficulties, other methods depending on quite different principles have, as before intimated, been suggested and applied. The general principle involved is to derive the height from observations made at a single station, thus eliminating the second observer and the errors he introduces, putting in his place some more or less plausible suggestion as to the origin of the aurora itself. Galle, for instance, assumed that an auroral streamer is parallel to a free magnetical needle on the earth's surface, vertically below the beam. By observing the zenith distance of the auroral corona in his magnetic meridian, he obtained the angle made by his vertical with the parallel lines of light that compose the aurora, or the dip of the needle suspended in the region whence the light emanates. The magnetic charts show at what point on the earth's surface the needle would have the same dip. This gives a right-angled triangle whose base is known, and whose vertical side is the desired height. The weak point in the method is the assumption that the dip of the needle in the place where the corona is presumed to be coincides with that at the earth's surface immediately beneath it. Another method that has been applied is due to Bravais. It assumes that the auroral arch throughout its whole extent exists at a uniform distance above the earth's surface. If this assumption were justified, the determination of the azimuth and altitude of the two ends, and of the summit of the arch, would lead to a knowledge of its height. The method has been repeatedly tried with some modifications concerning the curvature of the arch, and of the position of the centre of the circle; but the very number of the variations that have been made condemns the accuracy and the applicability of the method. The observed apparent velocity of the motion of the arch, as seen from two stations in a magnetic meridian, has also been tried; and indeed, without further enumeration of the plans that have been suggested, one may say that the ingenuity and industry brought to bear upon this problem have been such, that if the definite beams and arches possessed a real existence and a definite locus, its solution would have been assured. That the parallax has remained so long indeterminate is probably due to the fact that the question has not been broached along appropriate lines. In his careful review, Prof. Cleveland Abbe makes some practical suggestions which, if applied, would go a great way to show how far optical illusion and perspective displacement affect this luminous phenomenon, which for so long has supplied poets with a simile for instability, and which under scientific examination gives additional point to the well-known lines of Burns:—

"Like the Borealis race  
That flit ere you can point the place."